

LETTERS TO THE EDITOR.

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The Tail of Halley's Comet on May 18-19.

PERHAPS the following observations I made of Halley's comet on the night of May 18, when it crossed the sun's disc, may be of interest as a record.

On that evening I crossed by steamer from Palermo to Naples, as I wished to have a clear horizon all round to see what would happen. The vessel leaves Palermo at 7 p.m. and arrives at Naples at 7 the following morning, and it seemed the best place for a view.

I may say that I had been watching the comet every night from May 7, and was quite familiar with its appearance. I say this because it was quite different from that of any other comet I can remember. The first time I saw the tail was when I came on deck on May 7 about 4 a.m.; the nucleus was not visible, but right across the sky was a long white streak just like a cloud, quite as opaque as a cloud, and I could not believe it was the tail of the comet at all; but on coming on deck the next morning at 2.15 I saw the same white streak, but this time with the nucleus, also of a very white colour.

I had no instrument with me to measure the length of the tail, but I got the quartermaster to lay it off on the ship's compass; he put it down on paper; it was E. $\frac{1}{2}$ N., and the end of the tail was E. by S. $\frac{1}{2}$ S., which is about $22\frac{1}{2}$ ° horizontal measurement; the real length of the tail itself I could only estimate as "about half-way across the sky."

On the night of May 18, as soon as it got sufficiently dark, the tail was plainly visible; there was a ten-days' old moon which rather interfered with the view, but about 2 a.m. it had got sufficiently low and behind a thin but convenient bank of cloud, so that it did no further harm to my observation. Of course there was no nucleus to be seen; that was down below with the sun, but the tail was quite different in character from that which I had seen on the previous nights. It was not a long streak of white, but a confused mass of pinkish light extending along the horizon for 40° or 50° , and then stretching right across the sky, coming gradually to a point at the wide naked-eye double star (α and β Capricorni) below Altair, in line with the three stars. The tail narrowed in on its course upwards, and passed just below the Great Square of Pegasus, γ Pegasi being well in the tail, but α Pegasi was clear of it.

I continued watching the tail for shooting stars in its neighbourhood, but I only saw three or four; there was nothing particular about them, except that they seemed to start from the edge of the tail, which was well defined, and only travelled 4° or 5° from it.

But there still remains a curious sight to describe which I saw on the other side of the ship.

About 2.15 a.m. I went aft to get the time from the chart-room clock, and, happening to look over the port side of the ship to the west, I saw a pillar of light on the opposite side of the earth to that from which the comet's tail came up; it was about 45° (roughly) high and 50° or 60° broad at the horizon; it was straight up and down, and was much brighter in the middle than at the sides, and the bright part seemed like a pillar of light, but the lighter and more transparent sides came up and formed a large cone. The setting moon was a good deal to the right of the cone, and was somewhat clouded out, and had no connection with it. At the time I took the cone to be the Gegenschein, and did not pay much more attention to it, beyond looking now and then to see that it was still there.

Both the cone and the tail were visible from 2.15 to 3.5 a.m. It is quite possible that at this time the earth may have been passing through some of the tail, and had divided it in two.

I was up at Monte Casino the next night; unfortunately a fog came down on the mountain, but I heard that at the observatory they had seen an arch of light over the

part of the horizon from which the tail came. I did not see this; but I was at sea-level, and the observatory is up some 1500 feet.

HOWARD PAYN.

20 Hyde Park Place, London, W., June 17.

AN observing party was organised at this college for the purpose of taking note of any physical disturbances which might occur during the passage of the earth through the comet's tail, particulars of which will be published later. Our object in now writing is to put on record a remarkable appearance which presented itself at about 3.30 on the morning of May 19.

The comet has been visible here to the unaided eye since April 12, and up to the morning of May 18 the tail presented what may be termed a normal appearance, i.e. smaller at the nucleus than at the extremity, but on the morning of May 19 the character had altogether changed. At about 3.30 a luminous patch was seen at an altitude of about 20° from the horizon, and in the place where the tail formerly appeared. There were some clouds near the horizon, and as these cleared away the whole of the tail became visible, extending at 4.30 right up to the zenith, and there being lost in the Milky Way.

When there were no clouds the sky was remarkably clear, the Milky Way shining most brilliantly. The light from the tail of the comet was polarised, but not so distinctly as was the case with the normal tail on previous mornings. The tail persisted until daylight. It, to some extent as regards shape, simulated the Zodiacal Light, but at the same time was essentially different, and did not appear in the usual situation of the light, as it was many degrees to the north of the sun. It was much longer, narrower at the base, and ten times brighter. There is no question but that it was the comet's tail.

At 4.30 the upper half of the tail was quite free from cloud, and the gradual narrowing towards the upper end was most marked. It seemed from the curvature of the edges that a portion was missing from the under side of the tail. The conviction was borne in upon us that we saw a portion of the tail blotted out or cut off in some way, and this was certainly not done by cloud. Was it done by the earth's atmosphere? The following morning was cloudy, and nothing was seen at 3.30, but the comet appeared in the western sky at 5.40 in the evening.

Observations were not taken on succeeding mornings, which perhaps was a mistake, as something may have been left behind after contact with the earth, if contact really happened.

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The Colour of Pure Water.

HAVING noticed the colour of the sky, of air, and of water under different conditions, I was reminded on reading the report of Lord Rayleigh's lecture (NATURE, March 10) of a few notes I had made from time to time, and now think they may prove of interest.

First, optically pure water cannot be obtained by distillation. Prof. Tyndall asked me to prepare some pure water for him, which I attempted, first by distillation with acid permanganate, and then re-distilling this from a copper vessel and collecting the liquid in a bottle placed in a large bell-jar of hydrogen, a gas which is known to provide an optically pure atmosphere. The resulting water was not optically pure. Pure water was prepared by Tyndall by melting clear block-ice in a vacuum. Its colour was blue when seen through a tube 3 feet long.

The colour of a hard water which has been softened by Clark's process may be seen at the Colne Valley water-works, visible from the train on the up line just south of Watford Station, and at Joynson's paper works at St. Mary Cray, in Kent. When the members of the Society of Chemical Industry visited these works some years ago, they were much struck by the very beautiful blue of the water. It was even suggested that it had been purposely coloured with a very pure blue dye. Water of similar purity, containing very little mineral matter, being remarkable for its softness, comes from the Greensand below the London Clay. Such blue water I have noticed

in the bed-rooms and bath-rooms of the Euston Hotel, the supply coming from an artesian well.

There are two natural sources of extremely pure water with which I am acquainted, and I cannot believe that the water from the second source receives any colour or appearance of blue through the reflection of light by fine particles in suspension. The first is in the Fairy Loch beside Loch Lomond, situated on a little promontory south of Tarbert. It is difficult to see that there is any colour in the water except at a point where it wells up from a fissure in the rock and passes over a vein of milk-white quartzite which crosses the bottom; here it exhibits a beautiful blue colour.

The second source is the Wells of Dee, situated in the Larig (Learg Gruamach) at the foot of Ben Macdui, and between it and Braeriach, about half-way between Deeside and Speyside. It is a small tarn or pool with a bottom like that of three miles of the pass—nothing but large pieces of splintered red granitic rock. It stands about 2700 feet above the sea. The water, according to my recollection, passes down underneath broken rocks in a narrow rift in the mountain side, and is derived from the melting of snow on its northern slope near the summit, which is 1598 feet (1500 feet by my aneroid) higher and above all vegetation. The pool is too small to be shown, but the stream which runs out of it appears on the Ordnance map (Sheet 64, 1-inch scale), springing from the highest point of the pass. Some small lochs on the opposite side of the pass, about $\frac{1}{4}$ miles further south, are also called Wells of Dee, and are the principal source of the river of that name. The bottom of the small pool is visible everywhere, and its apparent colour varies in proportion to its depth, being dull red near the sides, to a brownish-purple where it is apparently deepest. The pure blue colour of the water was only seen on putting a white object, such as a piece of porcelain, into it. The effect of the blue colour of the water on the light reflected from the red rock at the bottom is to give it a purple tint.

It is evident that the blue is wholly due to the absorption of rays of complementary colour, because if it were not the reflection of blue rays by suspended fine particles would be seen against a dark ground on looking into the water. As a matter of fact, the water when undisturbed on the surface was not visible; it was very difficult to form any idea of its depth, everything on the bottom being sharply defined. These observations were made under a diffused and subdued light in a very clear atmosphere, the light being of uniform intensity over the whole sky, which was entirely covered with small greyish clouds, no direct sunlight or blue sky being anywhere visible. A fact adverse to the view that the blue could be reflected light is that the light which escapes reflection has a reddish-golden colour. In a hazy atmosphere when the sun is low and we look towards it, we see the golden colour; in the opposite direction we see the blue opalescence. The white light from the sky traversed the water in two directions to the bottom, and then, by reflection, back again, and it is safe to say that these two opposite colours would neutralise each other.

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The Temperature Conditions in Clouds.

As one of those who expressed doubt as to the possibility of the existence of the temperature conditions in a cloud described by Prof. Rotch at Winnipeg, I have been greatly interested by the letters of Dr. Aitken and Mr. Palmer (NATURE, November 18, 1909; June 2, 1910), but the examples which they quote do not present the same difficulty as Rotch's result, nor do they explain it.

The increase of temperature at or above the upper surface of clouds, which Dr. Aitken mentions, has been frequently observed in kite ascents at various places, while the two examples given by Palmer are (1) alto-cumulus, a wave cloud of the Helmholtz type formed at a surface of discontinuity: the temperature decreases upwards in the cloud itself; (2) alto-stratus, a shallow cloud formed also at a surface of discontinuity; here, too, the temperature decreases upwards in the actual cloud. In neither case do we attribute the temperature peculiarity to the clouds, but regard the clouds rather as the result of the temperature conditions.

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Rotch, however, found that in a cumulus cloud, 2 km. thick, the temperature increased from the base upwards by more than 5° C., and the increase was most rapid in the lower part of the cloud.

Dr. Aitken suggests that the sun, shining on the upper part of a cloud already formed and warming it, would account for the phenomenon, or at least for inversions near the upper surface of a cloud; but if the sun raised the temperature of the upper part of the cloud, that part would be no longer in equilibrium with its surroundings, and would rise upwards. Its temperature would, in consequence, fall under ordinary conditions until equilibrium with the surrounding atmosphere again supervened. The sunshine could only result in an actual increase of temperature if there existed already above the cloud an atmospheric layer of higher temperature than that in the cloud itself.

Now if Rotch's cloud were formed by convection currents according to the generally accepted ideas, the summit would be initially at least 10° C. colder than the base, and consequently its temperature must have been raised 15° C. to bring about the observed state of affairs. It is not easy to imagine how this could be done without dissipating the cloud, because it is unlikely that a cloud 2 km. high would be formed by convection currents without the upper parts losing some of the water-vapour which they originally contained, and in the present instance evaporation would begin before the temperature had risen 10° C. Moreover, the ascent took place about 9 a.m. in May; while assuming that 35 per cent. of the incident sunshine is absorbed by the cloud (Abbott and Fowle found 65 per cent. reflected) and that no loss of heat by radiation occurred, it would take a twelve-hour day near the equator to raise the mean temperature of a hemispherical cloud 2 km. high by 9° C. It appears certain, therefore, that solar radiation incident on the cloud cannot account for the phenomenon.

The only reasonable explanation I can put forward is that air below and above an inversion surface is lifted bodily upwards sufficiently far for condensation to take place on both sides. The balloon ascent must have been made in a region of convergence, and the mechanism by which the conditions were produced appears to have consisted of a cold, damp easterly wind penetrating beneath a warm upper current from a more southerly point.

Cambridge, June 6.

E. GOLD.

The Fertilising Influence of Sunlight.

With reference to Dr. Russell's remarks on this subject in NATURE of April 28, I should like to remark (1) that my point was not so much that toluene removed toxic material from the soil as that it rendered it insoluble. The question of washing out material from the soil was not raised by me. (2) and (3) Dr. Russell seems to beg the question by taking "fertility" and "bacterial activity" as synonymous. He has not, so far as I can find, proved that the addition to partially sterilised soil of an aqueous extract—or of a portion—of an untreated soil increases crop production (in contradistinction to soil fertility as indicated by bacterial activity and ammonium-production). If such is found to be the case, it would certainly require further experiment before it could be explained on the toxic theory.

With regard to water cultures, in one experiment the water was boiled every two days in some of the cultures, while in others it was not boiled. At the termination of the experiment—two days after the last boiling—the bacterial contents were found to be (per c.c.):

In unboiled cultures ...	$\dots \begin{cases} 2500 \\ 2100 \end{cases}$	Mean = 2300
In boiled cultures ...	$\dots \begin{cases} 350 \\ 400 \end{cases}$	Mean = 375

The quantity of material precipitated by potassium sulphate from the two solutions was (per million):—

Unboiled solution ...	$\dots \dots \dots \dots \dots$	30
Boiled solution ...	$\dots \dots \dots \dots \dots$	30

It would appear that if this substance had been produced by the bacteria there ought to have been at least seven times as much produced in the unboiled as in the boiled solution, since the bacterial content of the latter was never more than one-seventh of the former, and must have been for most of the time almost nil.